

Impact Fees and the Composition of Local Government Spending*

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Abstract

Impact fees are charges on new development earmarked for designated capital infrastructure. I study how their adoption affects local government spending using annual financial reports for 354 Florida jurisdictions from 1996 to 2019. Comparing spending across six categories within jurisdictions and over time, I find that capital spending and debt service in the designated category rise by nearly \$3 for every \$1 of new fee revenue, relative to other categories. Capital spending increases initially and declines toward baseline by year six, while debt service remains elevated. These patterns are consistent with borrowing against anticipated fee revenues. The estimates show that adoption of an earmarked revenue instrument changes spending composition.

JEL Codes: H71, H72, H74, H77

Keywords: Tax earmarks, impact fees, local government finance, capital spending, fiscal federalism

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1 Introduction

State and federal governments restrict how local governments may spend their revenues. Such fiscal rules are meant to discipline local choices, whether by limiting the accumulation of debt or by keeping spending from being channeled toward special interests (Brennan and Buchanan, 1980). Whether they raise welfare is not obvious and depends on a prior question about local government behavior: fiscal rules may protect residents from a local government that overspends or misallocates, but they impose real costs on a government whose spending aligns with voter preferences. Evaluating these rules therefore requires knowing how much they change local spending and through what channels.

One common restriction is a tax earmark, a requirement that revenues from a particular tax be spent on a designated category. Because local governments can reallocate other revenues across spending categories, earmarked revenues need not translate into additional spending on their designated use. I study the adoption of earmarked impact fees and find that capital spending and debt service in the designated category rise by roughly three times the new fee revenue, relative to spending in other categories by the same government. The expenditure response is consistent with borrowing against anticipated fee revenues to finance capital investment.

Impact fees are one-time charges levied on new residential or commercial construction, calculated to cover the capital cost that the new development imposes on a designated public facility, such as a road network, a school, or a water system. They are substantial — Burge and Ihlanfeldt (2006) report an average fee of about \$5,500 per single-family home across metropolitan counties between 1993 and 2003, roughly \$9,100 in 2023 dollars — and Florida law requires that the revenue be “specifically earmark[ed]” for capital facilities benefiting new users. Each fee is therefore an earmark tied to one spending category.

I use annual financial reports for 354 Florida local governments from 1996 to 2019, which record revenues and expenditures by source and function. My empirical strategy exploits variation in the timing of fee adoption across jurisdictions and six spending categories. Government-year fixed effects absorb fiscal changes common to categories within a jurisdiction, while category-year fixed effects absorb changes common to jurisdictions within a category. The estimates therefore capture changes in spending in the designated category relative to other categories. Fee revenues average only 2% of spending in categories with fee programs, yet adoption produces a substantial shift toward capital spending and debt service in the designated category.

The expenditure decomposition shows distinct responses of capital spending and debt service. Capital spending rises initially and declines toward baseline by year six, while debt service in the designated category roughly triples and remains elevated. This combination is consistent with jurisdictions borrowing to finance capital investment around the introduction of fee revenues. County governments show larger expenditure point estimates than municipalities, although the municipal estimates are imprecise and I cannot reject equal effects across government types.

Florida’s debt approval rules provide a possible explanation for these patterns. Long-term general obligation bonds payable from ad valorem taxation generally require voter approval via referendum, while revenue bonds backed by non-ad-valorem revenues can generally be issued without voter approval. Impact fees may therefore support borrowing for capital projects that would otherwise require a referendum or another source of pledged revenue. If this explanation is correct, it suggests that referendum requirements impose a binding constraint on borrowing for capital investment, and that earmarked impact fee revenues may relax that constraint.

The roughly \$3 response combines both capital outlays and debt service payments. Over the six years following adoption, the cumulative estimated increase in these expenditures is roughly twice the cumulative estimated increase in fee revenue. Fee adoption brings an immediate increase in capital investment, followed by a decline toward baseline by year six.

These findings show that adopting an earmarked revenue instrument can change the composition of local spending even when its revenues are small relative to spending in the designated category. The accompanying debt service response suggests that financing decisions are part of this adjustment. For states authorizing local revenue instruments, the results highlight the importance of considering how earmarked revenues may interact with borrowing rules as well as spending restrictions.

2 Related Literature

I contribute to the large public economics literature on the fungibility of government revenues. Much of this work examines the budgetary consequences of intergovernmental transfers and finds robust support for a “flypaper effect”: jurisdictions spend the majority of transfer revenue instead of lowering taxes, violating what simple models of voter preferences would predict (Combs and Afonso, 2025; Feiveson, 2015; Dahlberg et al., 2008; Inman, 2008).

Related work extends this analysis to earmarked revenues. Tax earmarks are designed to affect both the level and the *composition* of government spending, so jurisdictions can respond along a second margin: increase spending on the designated program or divert those resources elsewhere. Earmarks are generally effective at tilting spending toward particular programs, with positive evidence on state lotteries earmarked for education (Evans and Zhang, 2007), gasoline taxes earmarked for highways (Nesbit and Kreft, 2009), county sales taxes earmarked for transportation and education (Afonso, 2015; Brunner and Schwegman, 2017), and development fees earmarked for capital spending (Jung et al., 2009).¹²

I extend this literature in three ways. First, I use variation in impact fee adoption timing across categories *within jurisdictions*, allowing me to estimate the causal effect of impact fees on the composition of local spending under the conditional parallel-trends and no-anticipation assumptions developed below. This identification strategy addresses a key limitation of prior work, which cannot separate the effect of earmarked revenues from contemporaneous changes in fiscal policy. Jurisdictions that adopt earmarked revenue instruments may do so in response to local economic conditions or as part of broader fiscal reforms (Baker et al., 2023), making it difficult to isolate the earmark’s own effect. I account for these confounding factors by comparing spending across categories within a jurisdiction.

¹Three of these studies are closest to mine. Jung et al. (2009) examine impact fees and special assessments across 695 American cities between 1980 and 2000 and find that fee use is associated with higher capital spending, but they measure spending at the city level rather than by function, so they cannot observe whether fee revenue raises spending in the category to which it is earmarked. Afonso (2015) and Brunner and Schwegman (2017) study local option sales taxes earmarked for transportation and for school capital projects, respectively, and both find spending responses that exceed the earmarked revenue. In each case the instrument is adopted for a single category, so identification rests on comparisons across jurisdictions and the estimates absorb any contemporaneous jurisdiction-wide fiscal change. Because the jurisdictions I study adopt fees in six categories at different dates, I can hold the jurisdiction-year fixed and estimate how spending changes across categories within a jurisdiction.

²A related literature finds similar results from revenue increases to single-purpose governments, such as school or water districts, which are effectively earmarked to a specific category (Brunner et al., 2022; Marchand and Weber, 2020).

Second, the expenditure decomposition provides evidence on how local governments adjust spending after adopting an earmarked revenue instrument. Several theories explain why earmarked revenues are not fungible across spending categories. A behavioral explanation holds that voters and the politicians who answer to them are susceptible to cognitive biases, such as mental accounting or labeling effects, which distort their spending decisions (Hines and Thaler, 1995). Under this view, earmarks affect spending even when they are not technically binding because decision-makers treat earmarked revenues as distinct from general revenues. Alternatively, Inman (2008) argues that the flypaper effect arises because voters are unable to write complete “political contracts” with politicians, who pursue their own agendas once in office and exploit information asymmetries to increase their budgets given the opportunity. Finally, institutional explanations emphasize how earmarked revenues interact with existing fiscal rules — such as tax and expenditure limits or debt approval requirements — to alter the set of feasible fiscal actions (Brooks and Phillips, 2010).

I find that capital spending rises initially while debt service in the designated category roughly triples and remains elevated. These patterns are consistent with borrowing against anticipated fee revenues and suggest a role for financing decisions in the spending response. They do not distinguish whether those decisions reflect fiscal rules, voter preferences, or political incentives. My work builds on Afonso (2015), who likewise finds spending responses exceeding earmarked revenues, by documenting the separate dynamics of capital spending and debt service following impact fee adoption.

These patterns arise for own-source earmarked revenues rather than intergovernmental transfers. One strand of the flypaper literature explains the apparent non-fungibility of federal grants through political contracting between a donor central government and recipient local governments: the donor implicitly conditions grants on spending levels that recipients cannot easily circumvent (Chernick, 1998; Knight, 2002). Because impact fees are levied and spent locally, this particular donor–recipient mechanism does not explain the spending response in my setting.

Finally, I provide staggered within-jurisdiction, category-level estimates of how impact fees affect government spending in their designated category. Prior work on earmarks has focused on other revenue instruments, such as sales taxes or lotteries (Evans and Zhang, 2007; Afonso, 2015; Brunner and Schwegman, 2017), or on aggregate spending effects rather than its composition (Jung et al., 2009; Nesbit and Kreft, 2009). Impact fees are unusual among earmarked revenue instruments in that they are doubly earmarked: first, to a specific category, and second, to capital expenditures (or debt service) rather than current spending. Following fee adoption, capital expenditures increase by up to \$16 per capita in the designated category, with an average annual effect equal to 36% of the sample mean over the six post-adoption years, and debt service payments roughly triple. These results document how spending adjusts along both the functional and expenditure-type dimensions of the earmark.

3 Institutional Background

3.1 Development Fees in Florida

Earmarked development fees are pervasive across the US (Gyourko et al., 2021). Development fees are assessed on new residential or commercial construction and are intended to finance the capital infrastructure necessitated by new development. Such fees can allow municipalities to achieve an optimal growth path by aligning the public and private costs of growth (Brueckner, 1997). This

efficiency argument holds only if fee revenues are spent on capital projects that benefit the new development generating the fee, rather than on services for incumbent residents or rents for public sector employees. The requirement that fees benefit new development also bears on their legal status ([Nollan v. California Coastal Comm’n, 1987](#); [Dolan v. City of Tigard, 1994](#)).

In Florida, impact fees are governed by strict legal constraints that include accounting and proportionality requirements. The Florida Impact Fee Act of 2006 formalized judicial precedent in requiring jurisdictions to “specifically earmark” fee revenues for use on “capital facilities to benefit new users.” Impact fees predate the Act by decades, and Florida courts had imposed earmarking and proportionality requirements well before it; adoption in my sample therefore spans the full panel rather than clustering after 2006. Both the state and courts enforce earmarking requirements: the state by monitoring earmarked revenues and local budgets, and the courts by hearing cases, generally brought by developers, regarding the imposition of impact fees and the use of their revenues.³

All forms of local government in Florida – counties, municipalities, and independent special districts – impose earmarked fees in a broad range of spending categories, including transportation, parks, and education. Roughly 90% of Florida’s counties and 70% of its municipalities generated some revenue from impact fees between 1996 and 2019.

3.2 Debt Approval Rules and Bond Financing

A key institutional feature of Florida’s fiscal framework is the distinction between general obligation bonds and revenue bonds. Long-term local debt payable from ad valorem taxation — that is, from the local property tax — generally requires voter approval via referendum under Florida’s constitution ([Florida Legislature, 2025](#)). Revenue bonds, by contrast, are backed by a dedicated non-ad-valorem revenue stream, such as an impact fee, a sales surtax, or state-shared revenues, and can generally be issued without voter approval. Earmarked impact fee revenues can therefore serve as the backing for revenue bonds, allowing jurisdictions to finance capital projects without the political cost of a referendum. This distinction helps to understand how earmarks might amplify spending: they provide not only direct revenue but also a way to bypass a potentially binding constraint on borrowing.

Because the pledged revenue stream depends on the pace of new construction, a revenue bond backed by impact fees exposes bondholders to the risk that development slows. This makes the financing channel most available where growth is expected to continue, consistent both with the larger estimates for counties and with the concentration of fee adoption during Florida’s construction boom.

³Courts have invalidated fees for violating these requirements. In 2021 the Home Builders Association of West Florida challenged Santa Rosa County’s school impact fee, contending that fees collected in one part of the county were being spent on schools serving a different area. The circuit court agreed, declaring the county’s impact fees “invalid and unenforceable.” <https://www.nahb.org/blog/2022/01/florida-hba-wins-school-impact-fee-case-that-has-statewide-significance>. Litigation of this kind polices the link between where fees are collected and how the proceeds are spent, which is the property my design relies on: a fee earmarked to a category is, in practice, spent in that category.

4 Empirical Strategy

My empirical strategy exploits the staggered adoption of earmarked impact fees across jurisdictions and spending categories in Florida. Jurisdictions adopt fees at different times and in different spending categories, creating a rich panel structure that allows me to estimate the effects of earmarked revenues on the composition of government spending under the conditional parallel-trends and no-anticipation assumptions described below.

4.1 Event Study Design

I first present a fixed-effects event-study specification to define the notation and describe the identifying variation. The estimates in the body use the local projections difference-in-differences (LP-DiD) estimator described in Section 4.2. Two-way fixed effects (TWFE) event-study estimates appear in the Appendix and are broadly consistent with the LP-DiD estimates.

$$Y_{ijt} = \alpha + \sum_{\substack{k=-T \\ k \neq 0}}^T \beta_k \mathbf{1}\{t = T_{ij} + k\} + \lambda_{ij} + \tau_{jt} + \gamma_{it} + \epsilon_{ijt} \quad (1)$$

where Y_{ijt} is the outcome of interest for government i in spending category j and year t ; T_{ij} is the year that jurisdiction i first adopts a fee in category j ; and λ_{ij} , τ_{jt} , and γ_{it} are government-category, category-year, and government-year fixed effects, respectively. The adoption year ($k = 0$) is the omitted reference period, so β_k measures the effect of adopting an impact fee on the outcome of interest in year k relative to the year of adoption. For never-treated government-category cells, all event-time indicators are set to zero. For treated cells outside the displayed event-time window, the displayed indicators are also zero.

The three-way fixed effects structure addresses multiple sources of potential endogeneity and confounding. Government-category fixed effects (λ_{ij}) account for time-invariant factors that cause governments to spend more on certain categories than others, such as local preferences for public goods, topography, climate, or institutional capacity. Category-year fixed effects (τ_{jt}) control for time-varying factors that affect all governments in a given spending category, including changes in federal or state policy, construction costs, demographic trends, or economic conditions that differentially affect spending categories. Government-year fixed effects (γ_{it}) are particularly important in this setting because they control for jurisdiction-specific shocks that affect all spending categories within a government, such as changes in local economic conditions, political leadership, or overall fiscal capacity.

The inclusion of government-year fixed effects ensures that identification comes from within-jurisdiction, cross-category variation in the timing of fee adoption. This addresses concerns that jurisdictions might simultaneously change multiple fiscal policies or that fee adoption might coincide with broader changes in local economic conditions. With these fixed effects, the parallel trends assumption requires that, absent treatment, the differential evolution of spending across categories within jurisdictions would have been the same for treated and control categories.

Equation (1) is a triple difference in structure. Identification compares the change in spending in a treated category against other categories in the same jurisdiction-year, and that within-jurisdiction contrast against the same contrast in jurisdictions that had not yet adopted a fee in that category.

I cluster standard errors by government throughout, allowing for arbitrary correlation across categories and years within a jurisdiction. Because municipal and county boundaries overlap, a given resident may be served by two jurisdictions that appear as separate clusters and whose fiscal shocks are correlated. Clustering by government does not absorb this dependence, so the reported standard errors may be somewhat understated.

4.2 LP-DiD Methodology

The two-way — or, here, three-way — fixed-effects specification in Equation (1) is inappropriate in settings with correlation between treatment timing and the magnitude of the treatment effect (Roth et al., 2023). Such correlation is likely in this context: treatment timing varies considerably across governments and earmarks and is correlated with jurisdiction and category size, suggesting that the timing may be related to how those fees affect local budgets (see Table 2). In the extreme case, this heterogeneity can lead to negative weights that reverse the sign of the estimated treatment effect.

My preferred estimates therefore come from the LP-DiD estimator of Dube et al. (2025), which addresses heterogeneity in treatment effects across adoption cohorts by implementing a “stacked” regression approach that pools clean control and treatment observations across multiple event studies. The causal interpretation of the LP-DiD estimates still depends on conditional parallel trends and no anticipation, which I discuss in Section 4.3. This method is particularly well-suited for this setting for several reasons.

First, the staggered adoption of impact fees across jurisdictions and spending categories creates substantial variation in treatment timing. Counties adopted fees earliest (primarily in the 1990s and early 2000s), followed shortly by municipalities. Within government types, fees for transportation and physical environment tend to be adopted slightly earlier than those for economic development or culture and recreation (Table 2). This variation in timing may correlate with the magnitude of treatment effects if, for example, early adopters face different fiscal constraints or growth pressures than later adopters.

Second, the LP-DiD approach allows for heterogeneous treatment effects across adoption cohorts while maintaining the event study framework. The estimator constructs a “clean” control group for each treated observation by excluding units that are themselves treated within a specified window around the treatment date. Under the parallel trends and no-anticipation assumptions, the LP-DiD estimator yields a weighted average of all cohort-specific treatment effects, where the weights are strictly positive and depend on the treatment variance and subsample size. Because my preferred specifications weight observations by population in 2000, the estimates should be interpreted as population-weighted LP-DiD effects that place more weight on larger jurisdictions.

The LP-DiD specification modifies Equation (1) by estimating separate regressions for each post-treatment horizon h :

$$Y_{ijt+h} - Y_{ijt-1} = \beta_h \mathbf{1}\{\text{TreatDelta}_{ijt} = 1\} + \tau_{jt} + \gamma_{it} + \epsilon_{ijt+h} \quad (2)$$

where $Y_{ijt+h} - Y_{ijt-1}$ is the change in the outcome from the year before adoption to horizon h , TreatDelta_{ijt} is an indicator for the year of treatment adoption, and τ_{jt} and γ_{it} are category-year and government-year fixed effects, respectively. The government-category fixed effects from Equation (1) are omitted: because the outcome is differenced relative to the pre-adoption year, including time-invariant unit fixed effects would impose government-category-specific linear time

trends rather than controlling for level differences. The coefficient β_h captures the average treatment effect h periods after adoption across all treated units. I define treatment adoption to be the first time a jurisdiction moves from zero to non-zero fee revenues.

To implement this estimator, I follow the algorithm in [Dube et al. \(2025\)](#) to construct a sample that includes newly treated observations and clean control observations. I define the treatment year as the first year a jurisdiction reports non-zero fee revenue in a given category. For each jurisdiction-category pair, I include observations in their year of treatment adoption (newly treated) and observations from never-treated or not-yet-treated periods (potential controls). In the baseline six-year specification, I exclude contaminated control observations – those that will be treated within the next six years – to ensure control units provide valid counterfactual trends throughout the post-treatment estimation window.

As in [Dube et al. \(2025\)](#), I exclude always-treated jurisdictions (those with fees in the first year of the sample) since they cannot provide a valid counterfactual under treatment effect heterogeneity. All specifications cluster standard errors by government and weight observations by their population in 2000.

4.3 Identification and Threats to Validity

Identification of causal effects relies on the assumption that treatment timing is exogenous conditional on the included three-way fixed effects. Several institutional features of impact fee adoption support this assumption. Florida’s impact fee legislation provides a common legal framework across all jurisdictions, and fees must be calculated based on standardized methodologies that link fee amounts to measurable infrastructure costs.

Adopting a fee requires a formal process: a jurisdiction must commission a study calculating the infrastructure cost attributable to new development, hold public hearings, and pass an ordinance. This process takes months and is observable to local officials in advance, which raises the possibility of anticipatory spending changes, such as deferring routine maintenance on an aging school building in anticipation of building a new school financed by fee revenues. However, I find no evidence of differential pre-trends in capital spending or debt service ([Section 6.4.3](#)).

A few threats to identification remain. First, fee adoption might correlate with unobserved local economic shocks that independently affect spending patterns. If jurisdictions adopt fees in anticipation of growth spurts that would naturally increase infrastructure spending, the estimated effects might reflect these underlying economic trends rather than the causal impact of earmarking. The government-year fixed effects partially address this concern by controlling for jurisdiction-wide shocks, but category-specific effects of local economic conditions could still bias estimates. A residential construction boom, for example, raises demand for road capacity and school seats more than for library services, so a jurisdiction-wide shock can have category-specific spending consequences that the government-year fixed effects do not fully absorb.

Second, jurisdictions often adopt impact fees in multiple categories simultaneously, which could create spillovers between treated categories within the same jurisdiction. If the new revenue relaxes the overall budget constraint enough that a jurisdiction also raises spending in categories without a fee, those categories are no longer clean controls and the estimated treatment effects are biased downward.

5 Data

5.1 Sources and Sample Construction

The unit of observation is a jurisdiction-category-year. A jurisdiction is a county or a municipality, and a category is one of six spending functions defined below, so a single county contributes six observations per year. I use administrative data on impact fee revenues and government expenditures. The source data cover all counties and most municipalities in Florida. The raw data span 1996 to 2019, coinciding with a period of rapid growth in the use of earmarked fees across the state. The primary data source is annual financial reports that all Florida jurisdictions above a low revenue threshold must file with the state Department of Financial Services. These reports provide detailed information on revenues and expenditures by source and function and follow standardized accounting classifications, allowing me to match fee revenues to specific spending categories. Florida school districts are not included in these financial reports, so I supplement the main data with educational expenditures from the National Center for Education Statistics.

I restrict the analysis to the six most common uses of impact fee revenues: transportation, public safety (police and fire), culture and recreation (parks and libraries), physical environment (water, sewer, solid waste), economic development, and education.⁴ The included categories comprise 94% of all reported fee revenues and 75% of all local government expenditures over the sample. I require that jurisdictions have reported revenues and expenditures for every year, yielding a final sample of 354 jurisdictions: 65 counties and 289 municipalities.^{5,6} Jurisdictions in the sample account for 80% of all local government expenditures in the included categories over the sample period.

These data record fee revenues rather than fee ordinances. I observe how much a jurisdiction collects in each category each year, but not the fee schedule itself. I therefore define adoption as the first year a jurisdiction reports non-zero fee revenue in a category, which is the date collections begin rather than the date the policy is enacted. This has two consequences. First, units are considered treated only after they collect fee revenues, which is the relevant date for spending outcomes because a pledged revenue stream must exist before it can back a bond. Second, because fee collection typically lags the announcement of the fee, any anticipatory response would appear in my pre-treatment coefficients; I find no differential pre-trend in capital spending or debt service. The absence of fee schedules also means I cannot estimate effects of fee size, only of fee adoption.

5.2 Key Variables and Summary Statistics

Table 1 presents summary statistics for revenues and expenditures by spending category. The data indicate clear patterns of specialization across jurisdictions and highlight the role of impact fees in government budgets.

Municipalities lead spending on public safety, physical environment, and culture and recreation.

⁴School districts in Florida occupy the same areas as counties and have independent taxing authority. However, impact fees earmarked for education must be approved and administered by the county, so I treat education as a county function in the analysis. I match county education fee revenues to expenditures for the school district coterminous with that county; municipal education fee revenues do not enter the education-category analysis.

⁵I classify the consolidated city of Jacksonville as a county due to its large size – it is twice as large as the next largest city in Florida – and the fact that the city assumes all regular county functions.

⁶I exclude the city of Moore Haven, with a population less than 2,000, due to its unusual role as guarantor for a national bond-issuing authority called CaPFA that finances projects across multiple states, resulting in extraordinarily high debt service payments per capita. More details at <https://capfa.org/>.

These services – which include police, fire, water and sewerage, parks, and libraries – tend to provide highly local benefits, so spillovers are relatively unlikely. Such services may be efficiently provided by small, specialized governments. In the analysis, all school-district expenditures are assigned to counties, which also account for the majority of transportation spending. Education and transportation provide benefits across larger areas and are hence more efficiently provided by larger governments (Oates, 1972).⁷

Table 1: Per capita fee revenue and expenditures

Earmark	Revenue		Expenditures					
	<i>Fee Rev.</i>		<i>Capital</i>		<i>Current</i>		<i>Debt Service</i>	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
<i>City</i>								
Cultural/Rec	1.4	5.6	56.2	145.4	198.1	161.2	8.8	46.3
Economic	0.2	2.4	17.8	67.2	52.7	85.8	4.1	24.4
Physical Env.	6.4	21.3	31.3	115.5	696.4	798.0	35.1	72.9
Safety	1.8	12.6	33.2	43.4	688.6	511.2	1.6	7.5
Transportation	3.6	14.9	53.1	76.6	107.7	102.4	4.2	17.1
<i>County</i>								
Cultural/Rec	2.5	4.8	31.5	32.2	75.5	41.8	2.7	7.9
Economic	0.0	1.3	5.5	12.4	54.5	63.4	0.7	4.3
Education	10.6	18.8	201.5	139.1	1360.3	266.1	41.1	21.7
Physical Env.	4.0	15.2	17.9	42.1	328.9	447.7	26.0	77.1
Safety	1.4	3.7	27.3	24.6	517.3	206.4	2.0	10.5
Transportation	11.8	19.6	73.9	79.2	362.0	254.4	4.8	20.6

Notes: Summary statistics for local governments in Florida between 1996 and 2019. Statistics are weighted by population in 2000 and deflated to 2023 dollars. Fee revenue is earmarked impact fee revenue per capita.

Most fees are earmarked for capital-intensive categories. Municipalities use fees to pay for upgrades to utilities, such as water and sewerage systems, and to improve transportation infrastructure. Likewise, most county fees are earmarked for education or transportation projects. Fees for public safety and for culture and recreation are much less common, despite the fact that these categories account for large shares of government expenditures, suggesting that such services may be relatively labor-intensive and do not require large capital investments.

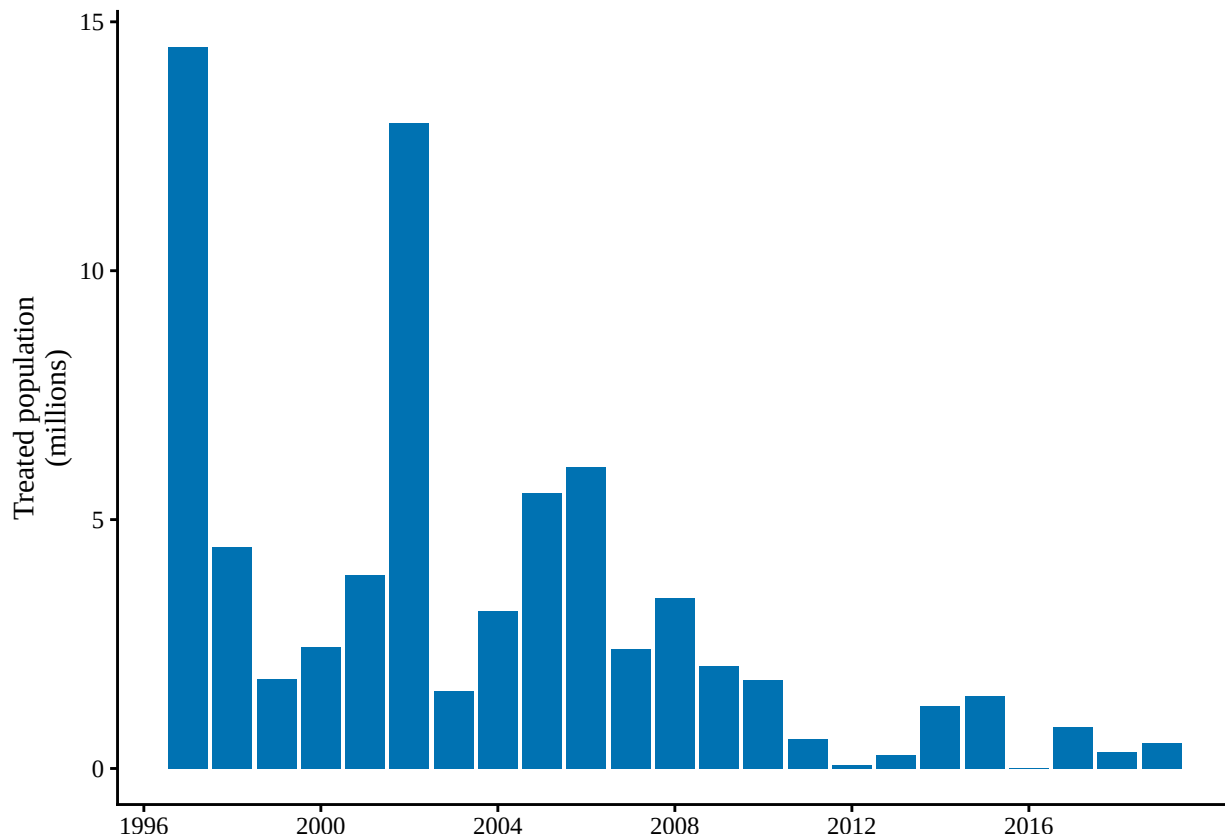
Earmarked fee revenues are substantially lower than category expenditures. Among jurisdictions with fee programs, fee revenues comprise an average of 2% of total category spending. This small revenue share suggests that a simple minimum-spending constraint may not bind mechanically in many cases, since fee revenues are small relative to existing category spending, though the data do not separately identify the discretionary non-fee spending that could be reallocated.

⁷In Florida, as in most of the South, school districts emerged during Reconstruction and typically adopted county boundaries to accommodate rural, low-density populations and to take advantage of existing county infrastructure.

5.3 Treatment Timing and Variation

Figure 1 reports population-weighted impact fee adoption events by year, counting residents once for each adopting jurisdiction-category event. The adoption pattern reveals several distinct waves of fee implementation, with the largest increases among analyzed treated units occurring in the late 1990s and early 2000s, coinciding with periods of rapid population growth and development pressure in Florida.

Figure 1: Impact fee adoption by year



Notes: Bars show population-weighted adoption events: the total number of individuals living in jurisdictions that adopted an impact fee in a given jurisdiction-category-year. Individuals may be counted more than once if two or more overlapping jurisdictions adopted a fee in the same year or if a single jurisdiction adopted fees in multiple spending categories.

Table 2 shows the timing of fee adoption separately for each government type and spending category. Counties tended to adopt fees slightly earlier than other government entities, with most county adoptions occurring in the 1990s and early 2000s. Municipalities show more dispersed adoption timing; municipal median treatment years are generally around 2006, with a median of 2007 for economic development fees.

There is less variation in treatment timing across categories. Economic development fees are relatively uncommon and tended to be introduced slightly later than other fee types, though fee adoption generally happens over the full length of the panel.

Table 2: Treatment timing by government type and spending category

Unit Type	Earmark	Treatment Year			Total Units (#)		
		Min	Median	Max	Treated	Control	Always
City	Cultural/Rec	1997	2006	2018	80	162	47
City	Economic	1998	2007	2019	24	264	1
City	Physical Env.	1997	2006	2019	126	123	40
City	Safety	1997	2006	2018	83	170	36
City	Transportation	1997	2006	2019	81	173	35
County	Cultural/Rec	1997	2005	2010	25	28	12
County	Economic	2000	2004	2019	8	56	1
County	Education	1998	2004	2009	37	28	0
County	Physical Env.	1997	2004	2015	29	31	5
County	Safety	1997	2004	2009	32	14	19
County	Transportation	1997	2006	2015	21	19	25

Notes: Treatment year is defined as the first year a jurisdiction goes from zero to non-zero revenue from an impact fee in a given spending category. *Always* denotes the number of jurisdictions with fees in the first year of the sample (1996) that are excluded from the analysis.

6 Results

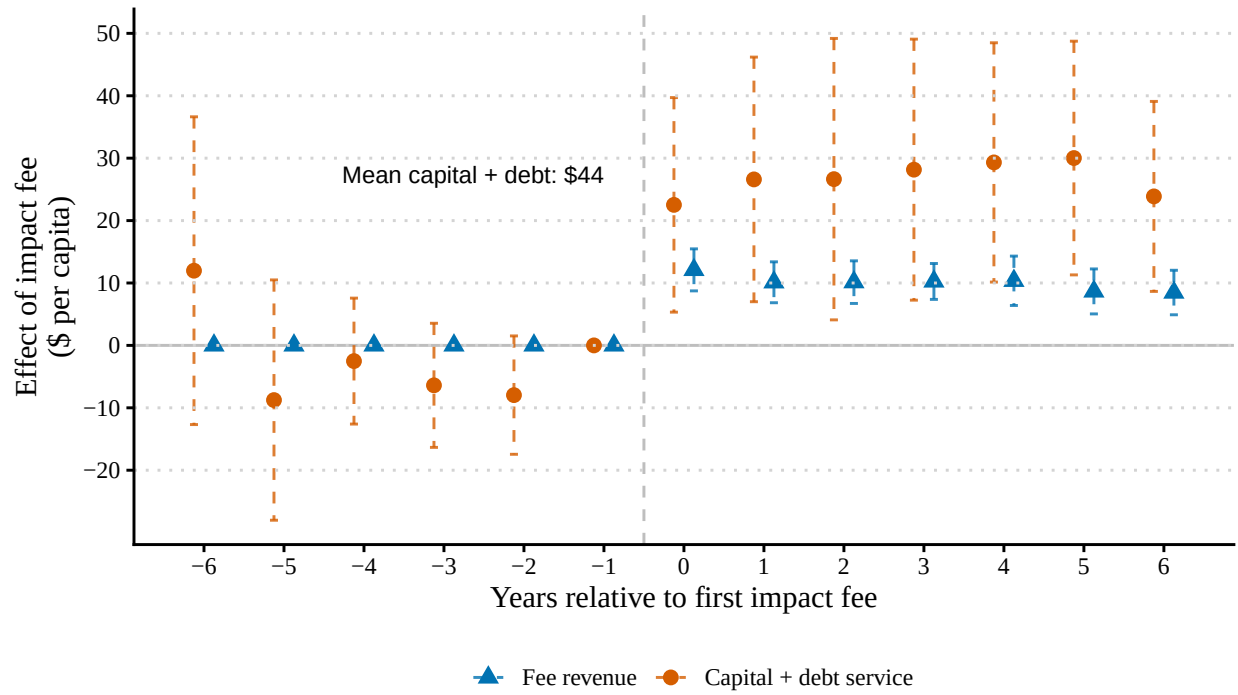
6.1 Main Effects on Revenues and Expenditures

I report heterogeneity-robust results from the [Dube et al. \(2025\)](#) LP-DiD estimator as the baseline and include the traditional event study results in the Appendix. The estimates from the two specifications are broadly consistent. Figure 2 shows the dynamic effects of impact fee adoption on fee revenues and capital spending plus debt service in the designated category. Because impact fees in Florida are legally restricted to capital facilities and associated debt service, this combined measure captures the spending types that are legally eligible uses of impact fee revenues within the designated category.

The revenue effects are precisely estimated and indicate an increase in fee revenues of about \$10 per capita that persists six years after the initial introduction. The immediate increase is a mechanical consequence of the way that treatment is defined. However, the dynamics are not: fee revenues in the designated category remain elevated compared to other categories in the same jurisdiction even six years after the initial introduction. Rather than imposing fees across multiple categories at once, jurisdictions generally impose fees in only one or two categories. The gradual dip in real fee revenues is consistent with fixed nominal fee schedules losing real value over time; later adoption in additional categories may also reduce the relative revenue difference between the initially treated category and other categories in the same jurisdiction.

Capital spending and debt service increase by nearly \$3 for every dollar of earmarked revenue. This amplification is inconsistent with a simple model where earmarks merely redirect existing revenues toward particular spending categories. Instead, the results suggest that earmarked rev-

Figure 2: Effect of adopting an impact fee on revenues and capital expenditures



Notes: The figure shows the dynamic effects of adopting an impact fee on fee revenues and capital spending plus debt service in the designated category from Equation (2). Coefficients are measured in real 2023 dollars per capita. All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

enues enable jurisdictions to finance new capital investment through debt issuance. I explore this mechanism through the expenditure decomposition in the next section.

Estimates from the dynamic TWFE specification, included in the Appendix, show qualitatively similar patterns, though with slightly smaller magnitudes and less precision (Figure 6).

6.2 Decomposition by Expenditure Type

To understand the dynamics of the capital and debt service response, I decompose spending in the designated category into its major components: capital spending, current (operating) spending, and debt service payments. Figure 3 shows this decomposition and provides evidence consistent with jurisdictions leveraging earmarked revenues to finance debt-backed capital investment.

Debt service payments in the designated category roughly triple following fee adoption, rising by approximately \$13 per capita from a baseline of approximately \$7. Category debt service payments are relatively low prior to imposing an impact fee, reflecting the preponderance of general obligation bonds – whose debt service is not tied to a specific spending category – in municipal and county finance.⁸ While individual point estimates are not always significant at conventional levels, the effect on debt service is economically large and becomes significant in some post-treatment periods. The timing of the increase, which jumps immediately after fee adoption, is consistent with jurisdictions arranging debt-backed capital spending around the introduction of the new fee revenues.

The effect on debt service payments is consistent with the institutional mechanism described above: under Florida law, long-term debt payable from ad valorem taxation generally requires voter approval via referendum, but revenue bonds backed by a dedicated non-ad-valorem revenue stream — such as impact fee revenues — can generally be issued without voter approval. Earmarked revenues may allow jurisdictions to bypass a potentially binding political constraint on borrowing, converting some projects that would require a costly referendum into a routine administrative action.

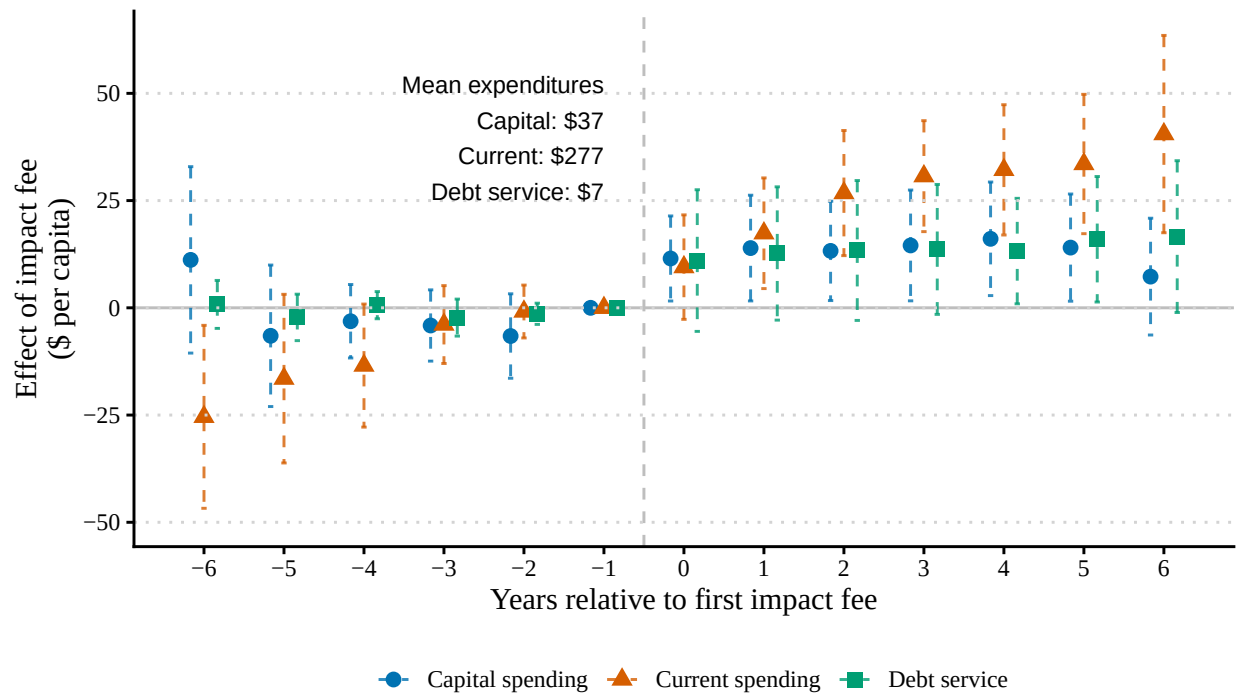
Consistent with the increase in debt service, capital spending also increases substantially. Per capita capital expenditures jump by approximately \$15 after imposing a fee, representing new infrastructure investment financed through the combination of fee revenues and debt. By year six, however, capital expenditures have begun to decline back to levels comparable to other categories. Over the entire event study window, cumulative capital expenditures are 17% higher among treated jurisdiction-category pairs than untreated pairs. Because capital spending is trending back toward the control mean at the end of the window, this cumulative figure represents the effect over the twelve years around adoption rather than a permanent increase in the level of investment.

Current (operating) spending also shows a positive effect, though these estimates should be interpreted with some caution given marginally significant evidence of a pre-trend ($p = 0.08$; see Section 6.4.3). If the estimate reflects a causal effect, it may capture the operating costs of newly constructed facilities, such as schools or police stations, financed with fee revenues. Current spending remains elevated even after capital spending returns to baseline.

These results clarify how jurisdictions increase capital spending and debt service by more than the immediate revenue from fees. Jurisdictions appear to front-load capital investment by issuing revenue bonds, then spread the financing costs over multiple years. Capital expenditures decline as the initial investment phase ends, while debt service reflects financing costs spread over later years.

⁸While the six included categories account for three quarters of all government spending, they account for only about a third of all debt service payments.

Figure 3: Effect of adopting an impact fee on expenditures by type



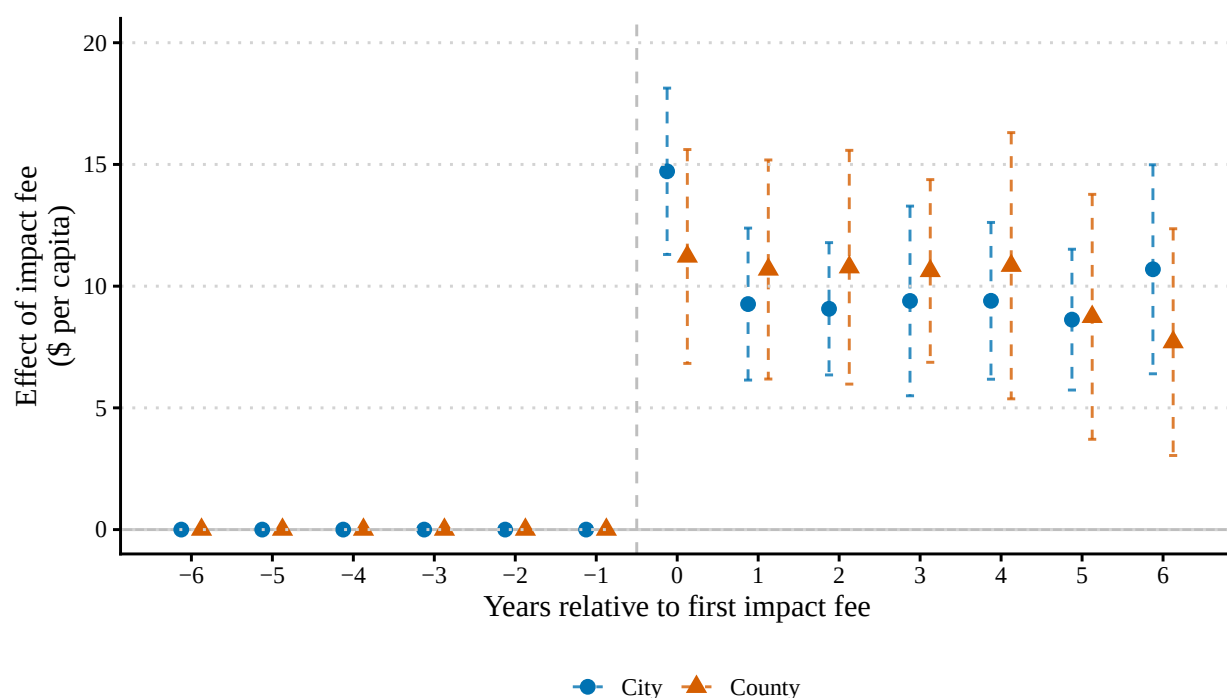
Notes: The figure shows the dynamic effects of adopting an impact fee on expenditures by type in the designated category from Equation (2). Coefficients are measured in real 2023 dollars per capita. All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

The estimated multiplier of nearly 3x on capital spending and debt service therefore combines new capital outlays with the financing costs incurred to front-load them.

6.3 Heterogeneity by Government Type

I next examine how the effects of impact fee adoption vary across different types of government entities. I estimate Equation (2) separately for counties and municipalities. The results, plotted in Figure 4, show that counties and municipalities both generate about \$10 of revenue per capita after introducing a fee, though revenues appear to decline slightly over time.

Figure 4: Effect of adopting an impact fee on revenues

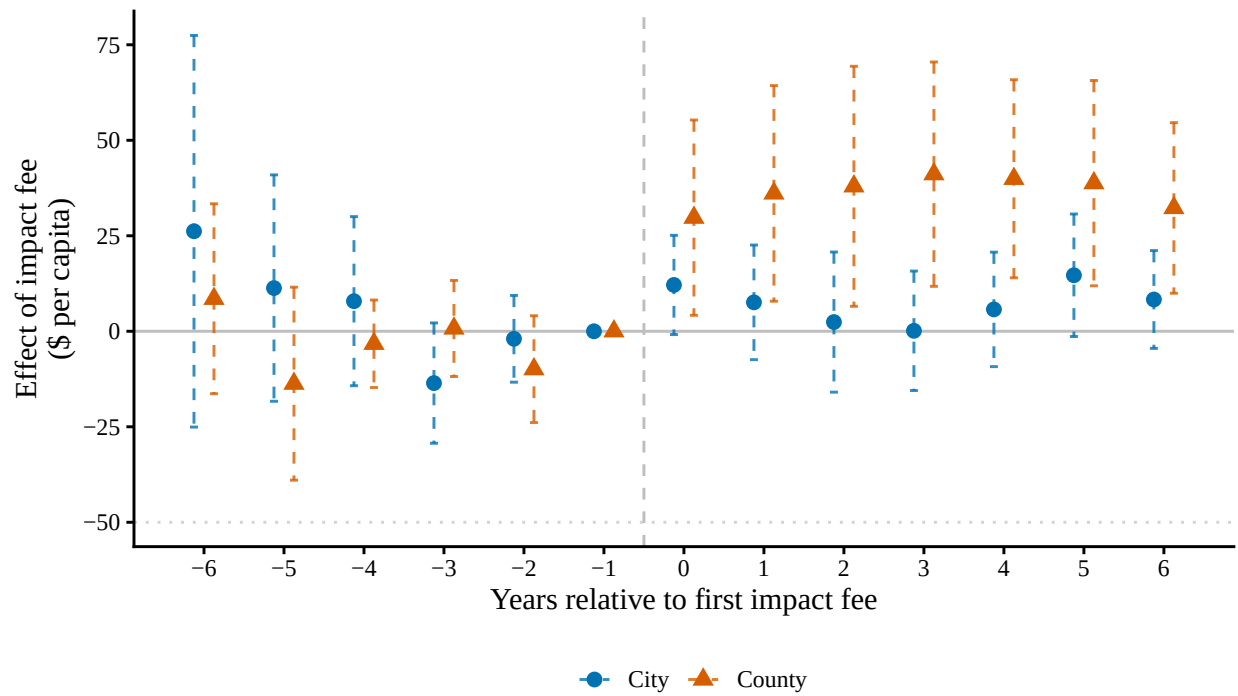


Notes: The figure shows the dynamic effects of adopting an impact fee on revenues separately by type of government from Equation (2). All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

Despite the similar effects on revenues, county expenditure point estimates are larger than municipal point estimates. County expenditures increase immediately and substantially following fee adoption, consistent with the debt financing mechanism described above. Point estimates for municipal expenditures are smaller and generally indistinguishable from zero, though I cannot reject that the effects are equal across government types. The pattern is consistent with the institutional explanation: county governments have greater access to bond markets, allowing them to leverage earmarked revenues through debt issuance.

While the heterogeneity across government types is suggestive but inconclusive, the patterns are consistent with the debt financing mechanism. Larger jurisdictions with stronger fiscal capacity and

Figure 5: Effect of adopting an impact fee on expenditures



Notes: The figure shows the dynamic effects of adopting an impact fee on expenditures in the designated category separately by type of government from Equation (2). All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

broader revenue bases are better positioned to issue revenue bonds backed by future fee revenues. Such jurisdictions may also face more binding constraints on alternative financing: securing voter approval for general obligation bonds may be more difficult in large, heterogeneous counties with diverse preferences than in smaller, more homogeneous municipalities.

6.4 Robustness

Placebo Treatment Assignment. Two potential threats to identification relate to the exogeneity of treatment timing. First, tax instruments tend to co-move within jurisdictions (Baker et al., 2023). If fees are introduced in the same year as property or sales tax hikes, the estimated expenditure effects could be biased upward due to income effects. While government-year fixed effects establish that category spending increases relative to other categories within the jurisdiction, designated spending categories might be luxury public goods that capture a disproportionate share of marginal tax revenues.

Second, jurisdictions often introduce impact fees in multiple categories in the same year. Such concurrent changes would cause a downward bias in the expenditure effects because spending in other categories would also increase after treatment, raising the average level of spending in control categories.

I conduct a placebo test to evaluate the magnitude of these threats. The placebo test features a randomization procedure that preserves the panel structure of the data while breaking the relationship between treatment (fee introduction) and financial outcomes. Specifically, I randomly assign the revenues and expenditures from each earmark to the treatment status of a different earmark from the same jurisdiction. For instance, if a jurisdiction adopted a transportation fee in 2003, the procedure might estimate the effects of adopting the transportation fee on education expenditures. I sample without replacement so that the new panel has the same composition of earmarks as the original data.

This randomization approach tests whether the observed expenditure effects could arise from general fiscal policy changes or economic shocks that affect multiple spending categories simultaneously. If the baseline results reflect concurrent changes in overall fiscal policy rather than earmark-specific effects, the placebo estimates should be similar in magnitude to the actual estimates. Conversely, if earmarking creates category-specific effects, the placebo estimates should be substantially smaller than the actual estimates.

I implement this placebo test across 500 randomization trials, estimating the LP-DiD specification for each trial and extracting the total expenditure effects in the second year after treatment. Figure 7 shows the distribution of placebo estimates compared to the baseline specification.

The results suggest that the estimates are not generated by shocks that raise spending similarly across categories within the adopting jurisdiction. The actual estimate of \$26 per capita falls in the upper tail of the placebo distribution. The observed expenditure effects are unlikely to be driven by concurrent changes in other fiscal policies or general economic shocks that affect multiple spending categories similarly.

Placebo Outcomes. I conduct a further placebo test by estimating the effect of introducing an impact fee on federal grant revenues. I find no evidence that fee programs led to increases in grant revenues (Figure 8). This exercise suggests that the timing of impact fee programs is not correlated with broader changes in the amount of intergovernmental transfers, ruling out correlated

federal grant revenue changes as an explanation for the expenditure estimates.

Parallel Trends and Other Robustness Checks. I find no evidence of differential pre-trends for the headline outcomes — capital expenditures or debt service — prior to the introduction of impact fees. A Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero yields p -values of 0.34 and 0.30, respectively. I also find no pre-trend for total spending ($p = 0.40$). However, I find marginally significant evidence of a pre-trend in current spending ($p = 0.08$), consistent with jurisdictions adopting fees in categories where demand for services is already rising. This motivates reporting capital spending, current spending, and debt service separately, while also indicating that category-specific demand growth remains a concern. Results are not sensitive to the length of the effect window (Figure 9).

7 Conclusion

Earmarked impact fees move resources into their designated category, and by more than the revenue they raise. The mechanism appears to be institutional rather than behavioral: earmarked revenues interact with debt approval requirements to unlock financing — specifically, revenue bonds that bypass voter approval — that amplifies the initial revenue into capital investment and the debt service financing it. The effectiveness of an earmark therefore depends on the institutional environment around it. In this setting the binding constraint is the referendum requirement on general obligation debt, and the earmark is powerful because it offers a way around that constraint; earmarks may have more muted spending effects in settings with different fiscal rules. These results suggest that state governments can influence local spending priorities by granting revenue instruments that interact with existing fiscal rules, offering a potentially powerful tool for managing local budgets under fiscal federalism. At the same time, states should be mindful of the potential for unintended consequences if earmarks enable local governments to issue excessive debt.

My results have potential implications for applications of the marginal value of public funds (MVPF) framework, especially for policy evaluation at the subnational level (Agrawal et al., 2022; Hendren and Sprung-Keyser, 2020). The MVPF approach aggregates fiscal externalities across jurisdictions by calculating net government costs as the sum of revenue and expenditure effects, implicitly treating all revenue sources as equally valued. However, my findings raise the possibility that fiscal externalities from earmarked revenues may not be valued like unrestricted general-fund dollars, while this paper does not directly estimate those marginal valuations. Consider a federal policy subsidizing electric vehicles that reduces state fuel tax revenues earmarked for transportation spending. If state governments place a low value on such earmarked revenues — perhaps because they already oversupply the designated category or face binding constraints on reallocation — the negative fiscal externality would be smaller than a standard MVPF calculation suggests (Hahn et al., 2024). This possibility implies that proper welfare analysis of intergovernmental policies may require weighting fiscal externalities by the receiving government’s marginal utility of different revenue types, not simply summing dollar amounts.

The non-fungibility of earmarked revenues may also help explain the relative decline of impact fees nationally between 2005 and 2018 (Gyourko et al., 2021), though my Florida estimates do not directly test this national pattern. Earmarked fees can impose administrative burdens on local governments because revenues are restricted to eligible capital uses and may be most useful when paired with debt finance for large, indivisible projects rather than directed toward a jurisdiction’s preferred spending mix. Such earmarked revenues may be relatively unattractive to local govern-

ments that have access to other, more flexible sources of revenue. As the tax base of development fees – the number of new housing units – declines across the US ([Glaeser and Gyourko, 2025](#)), the administrative costs of fee programs may begin to outweigh their limited benefits, especially if jurisdictions are unable to use earmarked revenues for operating expenses or maintenance of existing infrastructure.

A Baseline Results

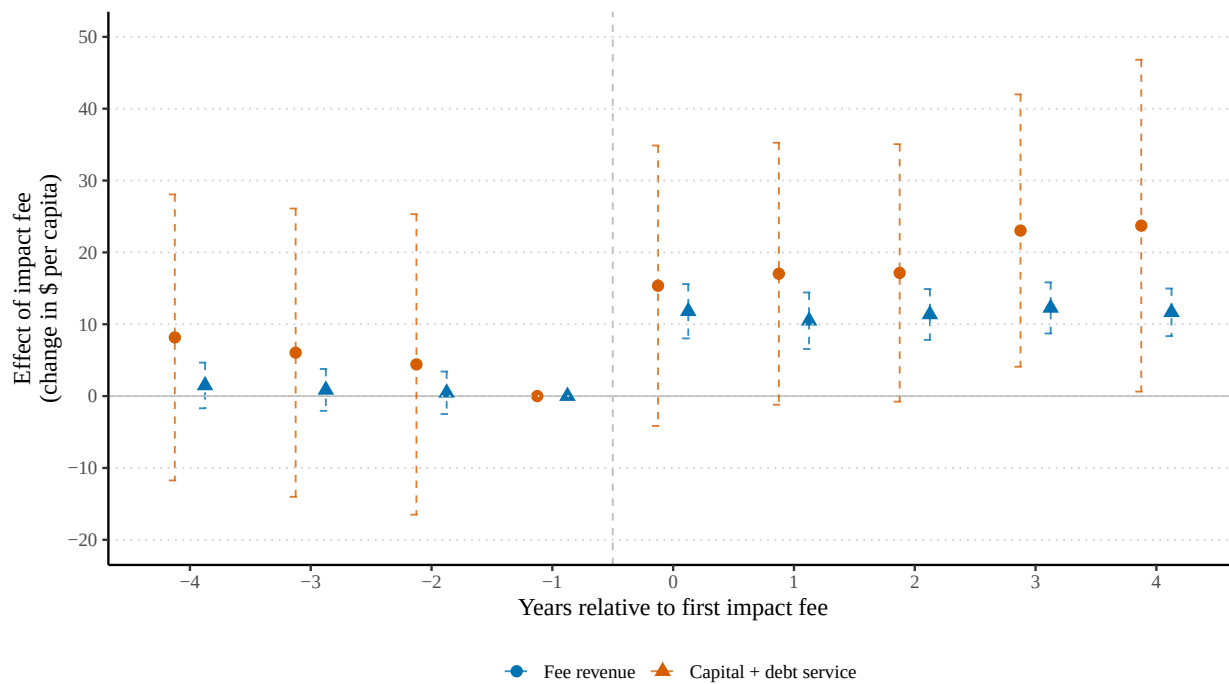
Table 3: Baseline LP-DiD results

Year	Fee revenue	Total spending	Capital spending	Current spending	Debt service
-6	0 (n/a)	-13.4 (-0.9)	11.2 (1.0)	-25.4 (-2.3)	0.8 (0.3)
-5	0 (n/a)	-25.3 (-2.0)	-6.5 (-0.8)	-16.5 (-1.6)	-2.2 (-0.8)
-4	0 (n/a)	-16.0 (-2.0)	-3.1 (-0.7)	-13.5 (-1.8)	0.6 (0.4)
-3	0 (n/a)	-10.3 (-1.4)	-4.1 (-1.0)	-3.9 (-0.8)	-2.3 (-1.0)
-2	0 (n/a)	-8.8 (-1.6)	-6.6 (-1.3)	-0.9 (-0.3)	-1.4 (-1.1)
-1	0 (n/a)	0	0	0	0
0	12.1 (7.0)	32.0 (4.7)	11.5 (2.3)	9.5 (1.5)	11.0 (1.3)
1	10.1 (6.1)	44.0 (4.8)	13.9 (2.2)	17.4 (2.6)	12.7 (1.6)
2	10.1 (5.8)	53.4 (4.8)	13.3 (2.2)	26.8 (3.6)	13.4 (1.6)
3	10.2 (7.0)	58.9 (4.5)	14.5 (2.2)	30.7 (4.7)	13.6 (1.8)
4	10.4 (5.1)	61.5 (4.1)	16.1 (2.4)	32.2 (4.2)	13.2 (2.1)
5	8.6 (4.7)	63.5 (4.1)	14.0 (2.2)	33.5 (4.0)	16.0 (2.1)
6	8.5 (4.7)	64.4 (3.8)	7.3 (1.0)	40.5 (3.5)	16.6 (1.8)

Notes: The table shows the dynamic effects of adopting an impact fee on fee revenues, total expenditures, and expenditures by type (capital, current, and debt service) in the designated category using the LP-DiD estimator in Equation (2). Coefficients are measured in real 2023 dollars per capita, and t-statistics are shown in parentheses. All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

B TWFE Results

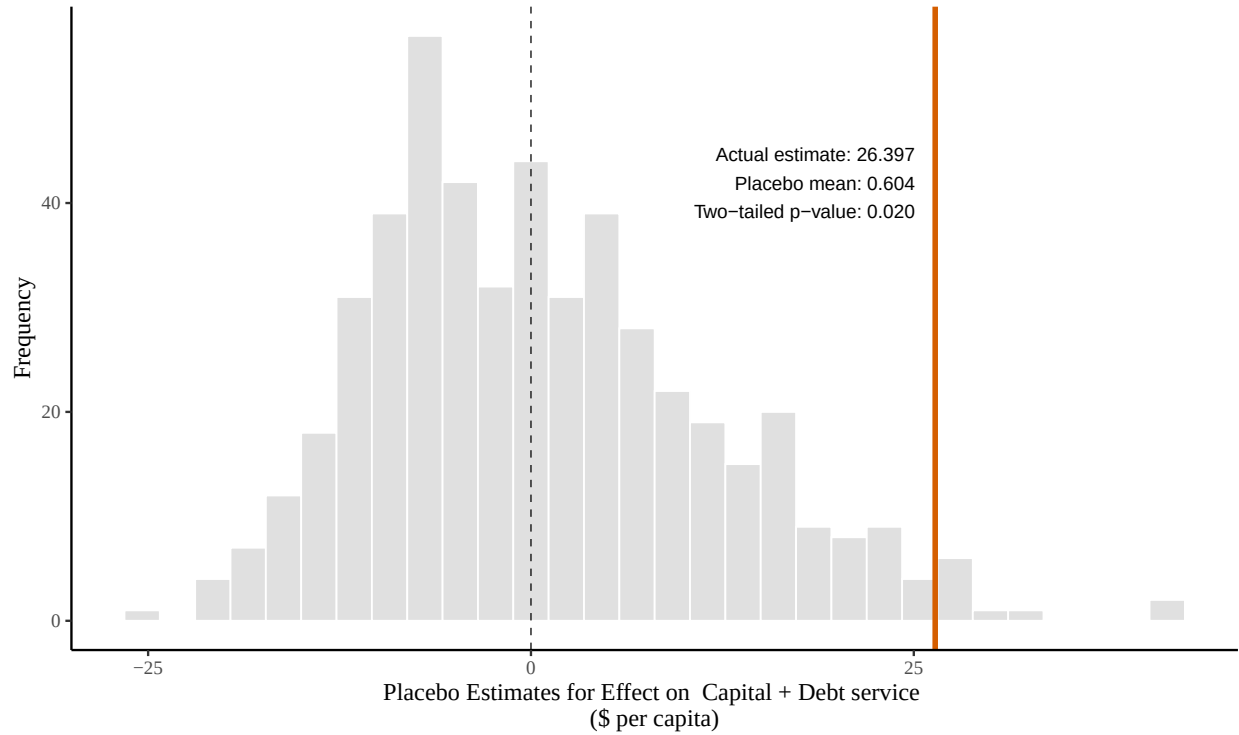
Figure 6: Dynamic TWFE effects of adopting an impact fee on revenues and expenditures



Notes: The figure shows the dynamic effects of adopting an impact fee on total revenues and capital and debt expenditures in the designated category using the two-way fixed effects estimator in Equation (1). All specifications include category-year, government-year, and government-category fixed effects, cluster standard errors by government, and weight observations by population in 2000.

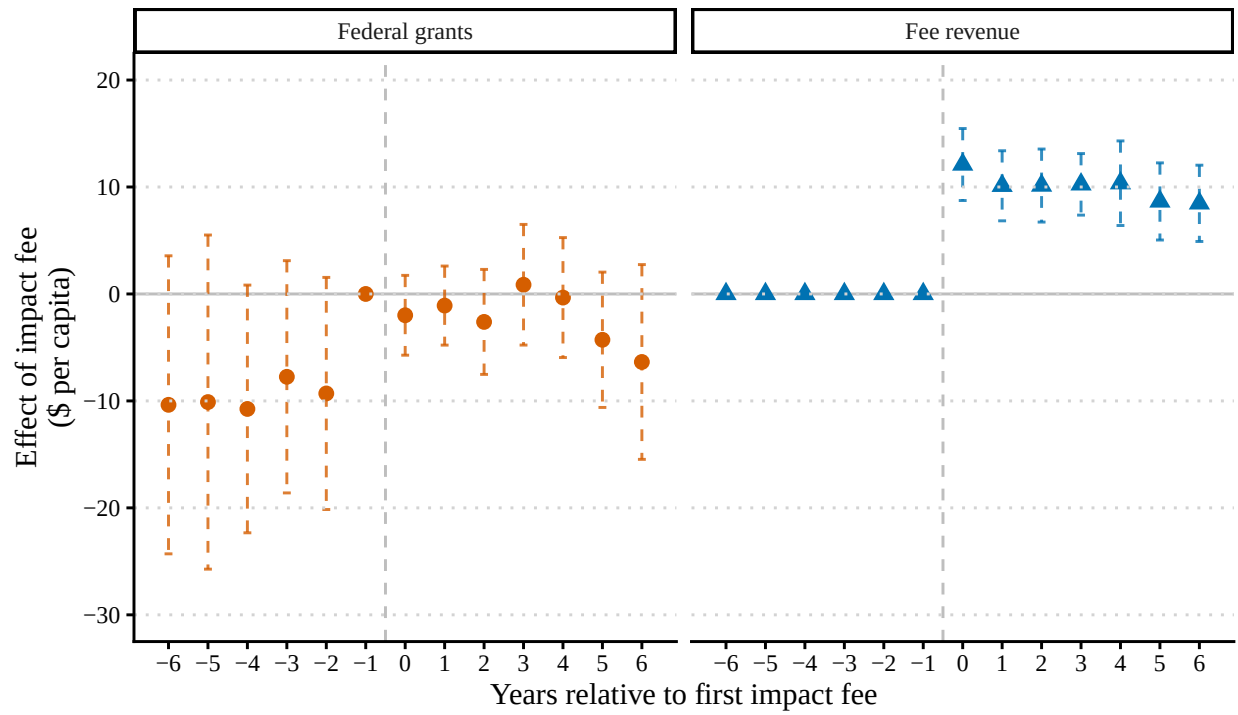
C Robustness

Figure 7: Distribution of placebo estimates



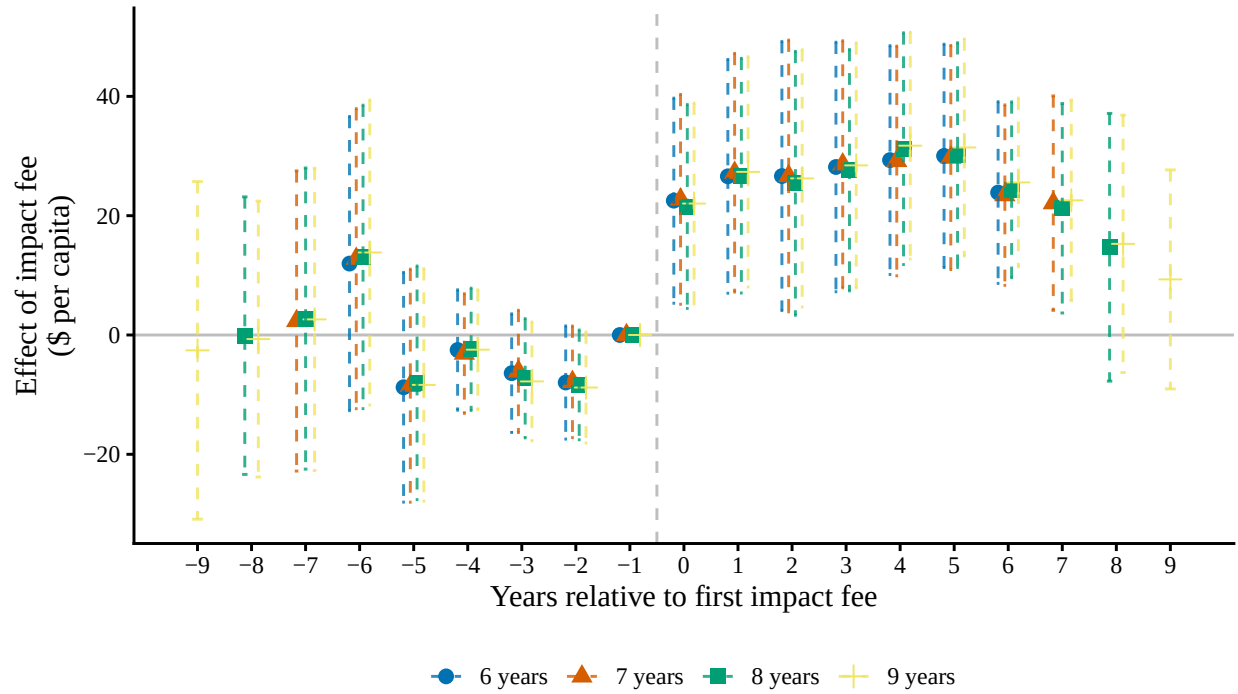
Notes: The figure shows the distribution of placebo estimates in the second year after treatment from the LP-DiD estimator in Equation (2) across 500 trials. All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

Figure 8: Effect of adopting an impact fee on federal grants



Notes: The figure shows the dynamic effects of adopting an impact fee on federal grant revenues from Equation (2). All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

Figure 9: Robustness to effect window: expenditure effects



Notes: The figure shows the dynamic effects of adopting an impact fee on capital and debt service expenditures in the designated category using the LP-DiD estimator in Equation (2) under six-, seven-, eight-, and nine-year effect windows. For each window, controls must remain untreated through the full displayed post-treatment horizon. Coefficients are measured in real 2023 dollars per capita. All specifications include category-year and government-year fixed effects, cluster standard errors by government, and weight observations by population in 2000.

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